monary vesicles, which are as simple as serous membranes. In conclusion, Dr. Williams thought they were indebted to Dr. Waters for his researches; but he did not consider them conclusive on this subject, interesting alike to pathology and diagnosis.—*Med. Times and Gaz.*, Dec. 23, 1865.

12. *Certain Forms of Hæmoptysis, unassociated with Pulmonary Tuberculosis.*—Dr. Richard Payne Cotton draws attention (*Lancet*, Dec. 2, 1866) to what he believes to be a not unfrequent, though little recognized form of non-tubercular hæmoptysis, met with chiefly in the female sex, but sometimes among males, generally in the early period of life. He relates three cases illustrative of this, one of which is the following:—

"A young lady, aged 18, recently arrived from a residence in one of the West India Islands, was supposed to be phthisical. I was requested to see her, and report upon the nature of her disease, about which several very conflicting opinions had already been given. She was anæmic, nervous, and out of health; had a dry cough, but had not become thinner; her catamenia were regular, but scanty; her appetite was capricious; and she had had frequent hæmoptysis, which, there was every reason to believe, did not proceed from either the mouth or fauces. The blood, upon examination, was found to be thin and watery, of a dark colour, free from coagulum, unassociated with either bronchial or salivary secretion, and in general appearance much resembling a mixture of *red-currant jelly and water.* I was informed that this was its general character. Sometimes it had been considerable—as much as half a pint in twenty-four hours; at others, it would not exceed a teaspoonful or two during the same period; sometimes it would be scarcely enough to tinge a pocket-handkerchief, and often it would disappear for days together. This state of things had existed for nearly two years, causing great anxiety to the patient and her friends, from a belief that it was indicative of pulmonary disease. Careful examination of the chest, however, failed to elicit any evidence that such was the case. Rest, change of air, and the tincture of sesquichloride of iron, entirely restored this patient to health. It is now more than three years since I was consulted, and I heard a short time back that the young lady was in perfect health."

"Other cases of this form of hæmoptysis," he states, "have fallen under my observation; but I shall not specially refer to them in consequence of not knowing their sequel. At least twelve or thirteen have happened in my own wards at the Hospital for Consumption. Two only of these were males; the rest were females, generally of delicate and nervous appearance, and under the age of thirty. Several had very suspicious symptoms of phthisis; but the physical signs failed to exhibit any evidence of pulmonary tuberculosis, and most of them improved in health under appropriate treatment. In every case the expectoration was of the same general character; sometimes it was mixed more or less with bronchial mucus, slightly tinged perhaps with blood, and sometimes with salivary secretion; but more frequently it was simply watery blood, resembling, as I have described, a mixture of red-currant jelly with water."

The following are Dr. C.'s conclusions, drawn from his observations of such cases:—

"1st. There is a form of true hæmoptysis in which the expectoration is of a dark colour, and of a more or less watery consistence, bearing a close resemblance to a mixture of red-currant jelly and water.

"2d. That such hæmoptysis is of non-tubercular origin, and may proceed from any part of the gastro-pulmonary mucous membrane.

"3d. That it is attributable to a morbid and fluid condition of the blood, allied, at least in appearance, to that which is met with in purpura and scurvy."

13. *Snow's Theory of the Causes of Cholera Explained and Illustrated.*—The following able exposition of Dr. Snow's theory of the propagation of cholera, given in the *Med. Times and Gaz.*, Oct. 1865, by Dr. B. W. Richardson, will be read with interest.

"The first element of the theory is to the effect that cholera, pathologically, is a disease exclusively of the alimentary canal; that the great primary change from healthy to diseased action is at first locally confined to the alimentary tract;

that on this there is rapid exudation of fluid matter from all parts of the body; and that the collapse, cramps, coldness, and other external symptoms of the malady are but results of the abstraction of water from the tissues.

"The second element in the theory is that the primary change in the alimentary canal is always induced by the introduction into the canal of a specific poison.

"The third element is that the poison itself is exclusively contained in the intestinal excreta of the infected person—that is to say, either in the vomit or in the matters passed by the bowels. There is nothing exhaled from the lungs, nothing from the skin of the cholera patient, that produces the disorder. It is all a question of the increase of poison in the alimentary canal, and ejection of poison from the canal.

"The fourth element in the theory is, that the poison is not a gas, is not a vapour, but a substance capable only of existence either in the fluid or the dried form of matter; *ergo*, it can only be wafted a little way by the air, and when it is in the dried state it can only be carried long distances by being attached to articles of clothing, or by being disseminated through the agency of water.

"These are the essentials of Snow's theory. They have been misunderstood because they have not been separated from certain broad and practical conclusions with which he carefully connected them, and which have been much misrepresented.

"The leading misrepresentation of the Snow theory is that he, Snow, connected the spread of cholera purely with the supply of impure water; and we constantly hear the theory spoken of as though the author of it, having no grounds to go upon, had a bare crotchet that because people drank bad water they took cholera. This is an utterly false view of the case. Snow only looked upon water as the great means by which the cholera poison was distributed. He said, and he proved, that if choleraic excreta be supplied to a community by and through the water which that community drinks, such process will prove the most determinate means of introducing cholera into the community, and that by such process great and sudden outbreaks of cholera will be secured. Acting on this thought, he actually advanced directly to the cause of the cholera during the terrible Broad-street epidemic in London, and, removing the handle of the pump by which choleraic poison was being dispensed to many hundreds of the population, he stopped the plague as if by magic. But while he held to this view of water as a means of propagating the poison, he enforced the opinion that there were other modes of transit. He puts these modes into four groups. First, he held that the moist cholera excreta on the clothes and bedding of infected persons might be carried mechanically by the *vapour* of water, and might enter the nostrils and mouth in that form, and so be swallowed with the buccal secretion. It was in this way, he believed, that laundresses engaged in washing the clothes of infected persons were so readily attacked. Secondly, he maintained that the poison might dry on infected clothing, and that from such clothing, on its being unfolded or moved, the solid organic matter might escape in small substance, might be wafted a few feet in the air, and might, in fact, be absorbed through the mouth by any one exposed to it. Thirdly, in respect of nurses, persons who lay out the dead, and others in attendance, he urged that these might actually carry the poison on their hands, and infect themselves by taking their food while their skin was not properly clean. In mines and other dark places where many persons congregate, this, he thought, was a very common means of communication. Lastly, he concluded that the very utensils—such as basins and cups used by the sick—might convey the choleraic matter, or even the cloths on which such utensils, imperfectly washed, have been dried.

"Thus there was not a process by which the matter of cholera could be conveyed, omitted by Snow in his thesis; and we venture to state that if all his precautions were carried out, cholera would soon be an extinct disease.

"Another cause of misapprehension of Snow's theory deserves notice and explanation. It was unfortunate that the author of the theory died while yet the question to which we have next to refer was under consideration. The question was, whether there exists a specific cholera cell capable of reproduction, and from the first of which all other cells proceed; or whether there can be such a thing as a spontaneous generation of choleraic poison. It must be admitted that Snow, who was a staunch opponent of the doctrine of spontaneous generation, held by the cell theory, and was at first content to deal with the difficulties it suggested by comparing the origin of cholera poison with the origin of organic forms. Thus when he was once asked at the Medical Society of London where the first cholera cell came from, he answered by begging the questioner to tell him where the first tiger or the first upas tree came from; adding, 'I have no power to answer questions on the subject of ultimate facts.' Later, however, in his life he was beginning to reconsider the question of the origin of the poisons both of cholera and of typhoid fever; and had he lived, we doubt not that, guided by his clear and vast intellect, and his unbiassed nature, he would have modified his views in accordance with the natural truths that might have been presented to him.

"Is there any reason why at this stage of our knowledge we should ourselves modify that part of the Snow theory which treats of cholera poison as a reproductive cell capable of development only by reproduction? We think there is, and we believe that, in fact, such modification widens and strengthens the basis of the theory. The position seems to be this, that all the points Snow originally taught—in reference to the alimentary canal being the seat of the disease, and the alimentary excreta the poison of the disease, as well as in reference to modes of transmission of the poison—are confirmed; but that, in addition, such poison may, under peculiar conditions, be produced without the intervention of a first case. It is not, however, probable, nor, as far as we can see, possible, for such generation of poison to commence in the living body itself. The evidence seems to point to changes occurring in excreta that have been passed from the body. These excreta, during a process of special decomposition, become transformed into poisonous matter, which by its presence tends to excite the same change, in continuation in similar matter, and which, introduced, by accident, into the alimentary system of the living body, excites a new organic secretion and reproduction of poison.

"That there are periods when organic matters in a state of decomposition undergo peculiar transformations, or, in other words, that the process of decomposition is not a uniform process, always accompanied by the same products, is now generally accepted by the physiological world as a truth the importance of which cannot be over-estimated. We shall hope, indeed, one day to show, on experimental demonstration, not only that organic matters yield particular products under particular conditions, but that those products are also particular causes of disease; and we believe that so far, and in accordance with this demonstration, the theory of Snow will have to be modified. We shall then, while retaining the essentials of his theory, accept them *minus* the dogma that there must be, it matters not how remotely, some connection between one case and another case. We shall say that a case being in a community will communicate the disease if it be permitted so to communicate, and that, too, whether the case be an importation or a development; but we shall not say that necessarily the first case was an importation.

"In so far as practice is concerned, the whole experience of the present epidemic has been to show that choleraic poison, once developed, travels only in the manner suggested by Snow, and that all rational measures for suppression of cholera rest on a correct appreciation of his theory. Thus, at Constantinople, infected articles of clothing were washed in water which afterwards escaped to a drinking fountain; thus, in Paris, the disease is spreading by the aggregation of diseased with healthy persons; thus now, as in all former epidemics, it runs that those who attend most closely to the sick and wash the infected clothing die most speedily; thus it is that when the poison is introduced into or developed in towns where it cannot contaminate great supplies of water, the disease is localized and limited.

"The most striking illustration of the truth of Snow's theory has been given us at Epping in the outbreak which we described last week. There eleven persons were attacked and eight died. There the centre of the disease was on elevated ground; so that a low position had nothing to do with it. There the disease occurred with a falling in the mean temperature, so that heat had nothing to do with it. There the disease struck and destroyed before the sufferers had time to feel dread; so that fear was not the cause. There eleven people were attacked, while hundreds in the district and some in the house escaped; so that no pervading atmospheric influence especially affecting *individuals* had anything to do with the disease. There there was plenty to eat and plenty to drink; so that poverty had nothing to do with the matter. There there were persons young and old, and of different occupations and sexes; so that age, sex, and habit had nothing to do with the event. And yet eleven persons were attacked. To what central and single source can we look for causation? It is before us. The sufferers drank from an infected well, or they came in contact with excreted matter from infected persons. It matters little whether Mr. or Mrs. Groombridge in their travels did or did not accidentally come in contact with excreted cholera poison, and did or did not communicate to the drinking-water of their house such poison: or whether that water, impregnated with organic matter derived from the closet of the house, became poisonous, under a special form of organic decomposition: the results are the same, and while we deplore the catastrophe at Epping, we cannot but express satisfaction that the outbreak occurred in a place from whence the choleraic virus could only be distributed amongst a limited number of the unfortunate."

14. *Efficacy of Lemon-Juice in Diphtheria.*—Lemon-juice has for some time been recommended in the treatment of diphtheria. M. GUERSANT was in the habit of prescribing to his patients slices of lemon to be kept in the mouth and frequently renewed; but this was only an auxiliary measure, being used together with cauterizations and astringent injections. But in a paper presented to the Academy of Medicine last June, Dr. RÉVILLOUT contends that lemon-juice is one of the most efficacious medicines which can be applied to diphtheria, and he relates that when he was a dresser, his own life was saved by its timely application. He employed three dozen lemons and gargled his throat with the juice, swallowing a little at the same time, in order to act on the more deep-seated parts. The results were that the false membranes were detached, the glandular enlargement decreased, and recovery soon followed. M. Révillout has noted eleven cases of entire success attained by this method of treatment. In one of his cases, which may probably be regarded as a type of the others, he prescribed as a gargle the juice of four lemons every hour, and the patient was directed to swallow a portion so as to modify the condition of the pharynx and œsophagus. The effects were to detach the diphtheritic exudations and to reduce the glandular enlargements, and the swelling of the face; and in twenty-four hours after the commencement of the treatment all appearance of diphtheria was removed.—*B. and F. Med.-Chir. Rev.*, Jan. 1866, from *Journal de Médecine et de Chirurgie Pratiques*, June, 1865.

15. *Hyposulphite of Soda in Diphtheria.*—Mr. J. C. MAYNARD extols (*Med. Times and Gaz.*, Dec. 30, 1865) the curative powers of the hyposulphite of soda in diphtheria. He ascribes the credit of this discovery to Dr. TUBBS, of Upwell.

The plan of treatment he describes as follows: "On first visiting a case, if not very far advanced, and in which only a few spots are visible, the throat is dressed twice a day with a strong solution of the hyposulphite of soda—viz., ʒiij of the hyposulphite; glycerine ʒij, with ʒvj of water. This generally removes the incipient exudation in forty-eight hours, sometimes in less. But if the case is an advanced one and the parasitic plant is making rapid strides, we wash the throat well out with warm water by means of one of Maw's flexible syringes. This is alike agreeable and most beneficial to the patient. The affected parts are then dressed with the strong solution, and a gargle of ʒss of the hyposulphite to half a pint of water, with ℥ss of glycerine, is given to be used every hour.

"The effect of the solution upon the exudation is most marked. It appears to solidify and dry up the false membrane, and when the syringe is again used, which is to be frequently done, the force of water will, if not completely, nearly entirely wash it away. The exudation in this way seldom or ever re-forms, and